In the mutual inter-diffusion without change of temperature of two gases of different densities through a fixed porous diaphragm, although no energy is withdrawn from or communicated to either of the gases, the rate at which single molecules take their equal measures of gas volume through the partition being very different, like their velocities, the measure of gas-volume which accumulates on the side of the denser gas soon raises the pressure there, increasing the intensity of the mechanical tendency $3pv$ on that side of the partition, while the same kind of mechanical tension diminishes on the other, and the temperature on each side of the partition is at the same time unaffected. As the porous diaphragm by its immobility (which prevents one of the sets of molecules from doing any work upon the other) resists the resulting force upon it, its counter-tendency is entirely derived from the increase of its own external virial, which has sprung up (at no expense of work, supposing the diaphragm to be perfectly rigid) in maintaining everywhere in spite of their impacts the common temperature or mean energy of both sets of molecules. Were the diaphragm away, it is evident that the rapid flow of rare gas-volume across the confines between the two gases towards the denser side would cause the centre of mass of the gas-layer, in which the mixture begins, to move bodily away from the denser gas, just as the diaphragm would do if it were free to move; and the *bodily motion* so given to the medial gas-layer will, as a form of external radiality in the layer, arising from the heterogeneity, require, in order to be constantly neutralised in the whole body of the mixing gases, such a redistribution of their temperature and density to be taking place at every instant throughout the two bodies of gas placed in communication, that their centre of mass as a connected (but otherwise *isolated*) system may never undergo any change of place during the mutual diffusion. The space originally occupied by the rarer gas will accordingly become the hotter, and that by the denser the colder portion of the whole volume which the gases continue to occupy when they are mixed.

It is in the same way that we can explain the action of regenerators in such air-engines as Stirling's and Ericson's, in passing through which gases change their temperature and volume (and therefore their tendency or "radiality," $E - 3pv$), at constant pressure, the counter-tendency being at the same time lodged or relaxed during the process in the regenerator, where it must be kept by non-conduction in the *tense* state (of actual heat-energy and virial combined) of "radiality" corresponding to the similar "heat-tension" of the gas by which its heat-energy exchanges are secured. The property of the usual non-conductivity required in the regenerator, is one of indifference of the molecules of a substance to the radiality (or to the sum of the sensible heat and the virial), of neighbouring molecules, or in which different values of the quantity $E - 3pv$ of small neighbouring parts of the substance equalise themselves with difficulty through the mass. But perhaps it is not the inter- but only the *intro*-molecular forces that furnish the radiality (and "virial") that determines the transmission of heat? If the former forces balance each other, which they do when the body is not vibrating by its elasticity, the virial of the intro-molecular forces only, together with the *vis viva*, may be "conservative" with regard to heat-energy, and may be employed in its transmission? Since radiancy of momentum is not heat-energy, we see that this natural effect of force radiality, or of virial and *vis viva* combined, can only be converted finally into actual heat-energy by some mechanism peculiar to the molecular structure of the solid and liquid bodies in which the heat-energy transmission takes place. Some kind of heat-engine apparently effects this process, for example, at the confines between the vapour and the liquid, when steam is condensed into water, but it is certainly a non-reversible one when the water-spray is colder than the steam which it condenses; and in the conduction of heat by solid bodies the process is also a non-reversible one; we only know the part which sensible heat, as temperature, or *vis viva*, acts in promoting heat conduction; and the virial by which it is perhaps also carried on, and which with *vis viva* conserves radiancy of momentum, may also be a fellow-regulator of the operation of which we have no certain knowledge, and over which we certainly have no direct control. But that it should invariably tend to lower the availability of heat, by heat conduction among the comparatively fixed molecules of liquid and solid bodies, will not, perhaps, when the internal motions of molecules are better understood, be more difficult to demonstrate from some theory of its action, than that it should sometimes serve to raise the availability of thermal energy by its action on heterogeneous gas masses. A. S. HERSCHEL

College of Science, Newcastle-on-Tyne

P.S. — Maturer reflection, since the first impression of surprise which Mr. S. Tolver Preston's announcement of the new experiment to which it relates caused me to express at some length, and perhaps unguardedly, in this letter has shown me that the properties of the virial, easily as they may conduct to some important results, do not, in this case, supply a complete solution of the problem of the final state of two gas masses at the same pressure but of two dissimilar densities on temperatures left to diffuse into each other in a confined space. The equal pressure on all parts of the inclosure predetermines the fixity of the centre of mass during the process, and consequently an unequal distribution of density, and therefore of temperature finally, when the mixture is complete; but the equation of the virial or the principle of conservation of the radiancy of momentum supplies no certain information what must be the law of this final distribution, one of its terms, $3pv$, or the virial of the inclosing pressure, being capable of undergoing unknown variations during the progress of the diffusion; and although the stationary condition of the mass at last implies that this term will not be permanently changed, yet both its value and that of the system's total moment of inertia round its centre of gravity (the *acceleration* of whose magnitude is the rate of change of the radiancy of momentum) may vary in the interval, with the result of leaving the latter moment of inertia permanently altered to an extent and in a way which cannot be defined. When in the simple case of a perfect gas the condition of the virial fails to afford positive information regarding the law of conduction and transference, or of rest and repose of heat in them under various distributions of temperature, it can hardly be expected that the same principle will furnish useful and definite results regarding heat-transmission through solids and other kinds of bodies of which the modes of molecular aggregation are almost totally unknown. A. S. H.

Time and Longitude

THERE is an old and instructive problem which I have lately propounded to several people, and have been struck by the great variety of answers given to it.

Although we often lose sight of the fact, it is nevertheless true that any given day or year does not begin all over the world at the same moment, but, commencing first at some point in the east, it travels round westward with the sun, so that two different years are often coexistent at the same moment, and it is easily possible for two events to occur a few hours apart, and yet that which happened first to occur in 1878, and the later event in 1877. In the same way each day of the week starts somewhere to the eastward of us and dies somewhere in the west. Taking, then, any given day of the week as Monday, the problem is—When and where did last Monday first commence, where did it end, and how long did it exist? Or, to put a similar question, Where did the year 1878 first commence, and at what Greenwich time?

I will simply state my belief that last Monday commenced in New Zealand somewhere about noon on Sunday, but not at noon, its commencement at that time and place being in no way connected with its position as our antipodes, but being a mere accident of civilisation. If the whole northern hemisphere should become civilised and inhabited, the day would then almost certainly commence at Behring's Straits, and would last forty-eight hours. A person crossing Behring's Straits-east or west would gain or lose a whole day just as he now does by sailing round the globe; so that he might easily cross over and spend a few hours of to-morrow with his friends and return in time for dinner, or might enjoy the New Year's Eve on two successive days.

If the Pacific Ocean became inhabited land, a meridian would have to be chosen as a starting point for the day, and a person stepping across this imaginary line would gain or lose a day. At the same moment that Sunday morning was commencing on the one side of this line, Monday morning would be commencing on the other, and there would be constantly two different days going on side by side with twenty-four hours' difference of time between them, though only a few yards apart. It would be possible for a person standing astride this line to have for an instant one foot in Monday morning, the other foot in Monday night, and his body in the previous Sunday.

I purposely avoid giving any reasons, and do not assert that all my views are correct, but I throw out the problem as an amusing one for argument and discussion, as it abounds in

© 1878 Nature Publishing Group

apparent paradoxes. At the same time it cannot fail to be instructive.
LATIMER CLARK

May 7

Cumulative Temperature

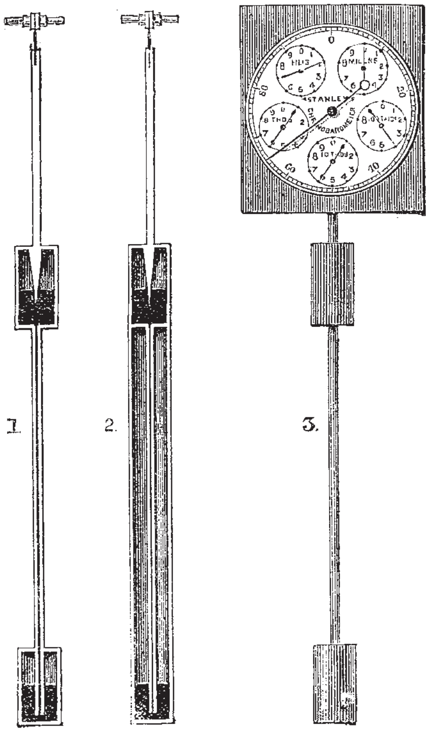
ATTENTION has been called in your valuable paper to the idea of registering cumulative temperatures by means of a pendulum, by M. von Sterneck, vol. xvii. p. 308, and this has called forth several letters. One gentleman has put forward my name as having devised means with some success. In an instrument exhibited at the Royal Society *soirée*, 1876, I could have left the matter resting at this point, but I am induced to write by the letter of your correspondent, "B," in vol. xvii. p. 486, who says, "The chief merit in this matter will belong to the person who puts the idea into a working form which can be proved capable of giving accurate results." As I think that I have fairly attained this end, or at least pointed out the way to it, with your permission I will describe the means which appears by the correspondence interesting to many of your readers. In my cumulative temperature clock the important element, the *pendulum*, is constructed as follows:—A steel cylindrical tube 32 inches long, 1¾ inch internal diameter, is hermetically closed at both ends. A rod is attached to one of the ends, which is placed uppermost, to connect this pendulum with the clockwork in the ordinary manner. An air-tight division is made across the tube or chamber at 5 inches from the upper end. A small tube leads from this division to the bottom of the chamber. A conical plug is inserted in the upper chamber, to be hereafter described. A screw plug is placed under the small tube in the outer tube to enable the upper chamber to be filled with mercury. When the pendulum is so constructed, the lower screw plug is removed, and the upper chamber and leading tube filled with mercury by means of a small funnel. In this full state the mercury is boiled, and the whole inverted. It then becomes a steel *barometer*. To convert it into a thermometer, a small air-hole is made in the outer tube (this is not shown in the engraving), and this hole is closed up with a small air-tight cock filled with a porous material. When this is screwed on and turned off, it is isolated from atmospheric pressure, and the mercury rises into the upper chamber by any increase of temperature causing expansion of air in the tube, and sinks in the same manner by loss of temperature, so that the pendulum becomes simply an air thermometer. The pressure of the air by expansion within the tube in the rising of the mercury changes the centre of oscillation of the pendulum and accelerates the clock, and *vice versâ*.

The clock is specially constructed to count beats only in units, tens, &c., up to ten millions, and the number of beats per day, week, month, or year, becomes the unit of temperature for the period. The exact length of time of each pendular oscillation being governed by the temperature at the time, the method becomes equal to one accurate observation at every second of time.

The difficulties of construction and refinement required upon this general description are of two kinds, mathematical and mechanical. The models that I exhibited at the Royal Society's *soirée* were imperfect, being of blown glass. The difference of oscillation per day for 1° Fahrenheit, was in these about 50, as taken at the Lambeth Observatory by the late Col. Strange. In the steel instruments described there would be about 100 oscillations additional per day for the rise of each degree centigrade. The mechanical difficulties are simply constructive. To obtain perfectly vacuum proof chambers, and to follow correctly the outline of the plug to be immersed in the vacuum-chamber. Also the adjustment of the correct volume of mercury, and the density of the contained air, by means of the cock, and the application of heat or cold to the outer case. The mathematical requirements are corrections. Thus: if the chambers were simply cylindrical, the mercury that rose by the pressure would have a different oscillation value for every point of space through which it rose. This might be corrected to equal scale value by making one or both the mercury-chambers conical, but it is much more simply done by inserting a conical plug in the upper chamber. There would also be a correction for the expansion of the mercury and the steel case, and from any irrationality in the expansion of the contained air. The whole of this correction being derived from heat might be made by one correction in the immersed plug. Prof. Stokes, Sec. R.S., kindly offered to calculate the exact form of this plug for me from data I was to supply. But I was ill shortly after this, and unable to attend to the matter, so I let it drop, but have the clocks and pendulums ready to complete some time hence.

I send a diagram engraving which shows the principle of the pendulum No. 2, for cumulative temperatures. No. 1 is for taking cumulative pressures upon the same system, if the science of meteorology should require such exact means of obtaining permanent records of pressure and temperature for long periods as for months or years.
WM. F. STANLEY

South Norwood, April 22



THE INTERIOR OF THE EARTH[1]

SIR GEORGE AIRY remarked that the nature of the subject was different from any upon which he ever lectured before, in regard to its indefiniteness and to the difficulty he should have if he considered it to be his duty to lead them definitely up to some point. He could only give them some idea of the theory to which he wished to lead them, and in doing so he would advert collaterally to a good many points which might be valuable. He proposed to divide his address into three parts. The first would relate to the measures of the earth; the second to observations on temperature; and the third to the manner in which they might suppose the earth to have been formed, especially with regard to the nebular hypothesis; and after that he would add some remarks on the conclusions to which these lead.

He described the process called triangulation, by which a large part of the contour of the globe is covered, and by which it is possible to lay down a map on which the distance between any one point and any other point is ascertained to within a few inches; how that this was valuable in ascertaining the dimensions and figure of the earth with the aid of the zenith sector, an instrument for measuring the apparent distances of stars from the point overhead. He showed on a large globe the principal lines of measurement which had up to this time

---

[1] Abstract of Address at the Cumberland Association for the Advancement of Literature and Science, by Sir George B Airy, K.C.B., F.R.S., Astronomer-Royal. Revised by the author.

© 1878 Nature Publishing Group